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ABSTRACT

A summary of current theoretical work in the area of concept formation is presented with emphasis on the presentation of thinking in theory. Examination of the logic relating thinking to behavior leads to criticism of the tendency to subordinate "behavior" to underlying processes. The difficulties inherent in analyses of this type are that underlying processes are commonly described in behavioral terms and that they are invisible and nonverifiable inventions of the theorizer. The present paper outlines the need for an explanation of behavior to account for its obvious generative and recursive character. Professor Bourne suggests that behavior may be a rule-following enterprise, and presents a brief review of research to illustrate his postulate. Finally, the place of thinking as a concept in psychology is discussed. Thought is seen as an event which reflects a change in the individual's possibilities for behaving. (Author)

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Occasional Paper No. 7

CONCEPT LEARNING AND THOUGHT

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PREFACE

In this Occasional Paper, Professor Bourne critically examines the most recent psychological explanations of thought, especially in connection with concept learning. He points to weaknesses in explanations of concept learning that follow solely S-R theory, S-R mediational theory, and cognitive theory, especially cognitive theory that attempts to explain concept learning in terms of underlying (nonbehavioral) mechanisms and processes. Rather, he views thought as behavior, closely linked to situations and overt actions. "Thoughts are events. . . . Like all events they are datable and locatable . . . thought is describable as a state of readiness . . . overt behavior can follow naturally, and the thought is part of that behavior. It must be clear that behavioral sequelae can be either explicit—an overt attempt at proper performance, or implicit—as, for example, in subvocal speech. But to recognize implicit behavior (thought) is not to endow it with the special property of control over overt attempts."

It is interesting to note that Professor Gary A. Davis in Occasional Paper No. 2 described two types of problem-solving behavior: covert trial-and-error behavior and overt trial-and-error behavior. Bourne's and Davis' independent analyses have much in common.

The Center is fortunate indeed that Professor Bourne was affiliated with the Center as a visiting scholar during the Summer Session of 1966. This theoretical paper and a subsequent technical report represent some of the contributions he made to the research program of the Center.

Herbert J. Klausmeier
Co-Director for Research

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The author is indebted also to Peter G. Ossorio, Maynard Shelly, Arthur W. Staats and Gary A. Davis, who read and commented critically on an earlier version of the manuscript. Many of the ideas reported here emerged from a series of discussions with Ossorio and Keith E. Davis. Their many contributions are acknowledged with appreciation and gratitude.

CONTENTS

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A summary of current theoretical work in the area of concept formation is presented with emphasis on the presentation of thinking in theory. Examination of the logic relating thinking to behavior leads to criticism of the tendency to subordinate "behavior" to underlying processes. The difficulties inherent in analyses of this type are that underlying processes are commonly described in behavioral terms and that they are invisible and nonverifiable inventions of the theorizer.

The present paper outlines the need for an explanation of behavior to account for its obvious generative and recursive character. Professor Bourne suggests that behavior may be a rule-following enterprise, and presents a brief review of research to illustrate his postulate. Finally, the place of thinking as a concept in psychology is discussed. Thought is seen as an event which reflects a change in the individual's possibilities for behaving.

I

INTRODUCTION

The alleged purpose of this paper is to show how theoretical and empirical research on human conceptual behavior has or might contribute to an understanding of thought. The task is formidable for many reasons, not the least of which is lack of clarity and agreement about the primary subject matter, thinking. Unless there is an acceptable answer to the question, what is thinking, there is no obvious way to proceed. But perhaps the more basic question is, would everyone recognize the right answer, the "true" answer, if there were one and it were given? If the answer to that is no, there is no point to proceeding.

The fact that we have a dilemma is disheartening, but that hasn't stopped many of us from talking about thinking, or from positing "thought processes" as the mechanisms which permit or enable organisms to do the complex things they do. And it is largely because of this that there is something to discuss in this paper.

The plan is as follows. First to be presented is a summary of some of the current theoretical work in the area of concept formation. Detailed accounts are available in a number of sources (Hunt, 1962; Kleinmuntz, 1966; Bourne, 1966a, 1966b) so the descriptions will be brief and cursory. Emphasis will be put on what might be described as the representation of thinking in theory, though there is no hope that the respective theorists would agree entirely with this distortion of their work. Second is an examination of the logic which relates thinking, as it is embodied in the theories, to behavior. Given that there are certain inadequacies in these formulations, the next step is to suggest an alternative which has implications not only for the psychology of thinking but also for the analysis of behavior. Some illustrative research will then be discussed in order to point up the possible uses of an alternative approach — which I should hasten to say is hardly new or original with me. And finally the paper returns briefly to "thinking" and its place as a concept in psychology.

II

THEORIES OF CONCEPTUAL BEHAVIOR

Contemporary theories about conceptual behavior are, for the most part, special cases of learning theory. While there is considerable variation, most of them are recognized easily as one of two main types.

S–R ASSOCIATIONAL THEORIES

Theories based on the principle of association formation describe a concept as a set of connections between some response and certain attributes or cues in stimulus patterns (Kendler, 1961). These S-R associations are assumed to gain strength gradually as a function of repeated and (relatively) consistent reinforcement of the correct response in the presence of the relevant stimulus attributes. Eventually associative strength becomes maximal and the concept is said to be formed. The associations are assumed to exist between only certain attributes of the stimuli and the response, so that these are the effective determiners of response. Irrelevant or unimportant attributes—those which vary from one instance of the concept to another—fail to develop any significant associative strength. Indeed some variants of this description (Bourne and Restle, 1959) propose a process of adaptation, concurrent with the development of associations, which results in the suppression of irrelevant cues (in a sense, S comes to ignore them). The proper associations develop in such a way that whenever the defining attributes occur, even in the context of a new stimulus pattern, the conceptual response will be elicited.

In most basic form, theories of this type make no assumptions about processes within the organism, save those which concern the existence of associations and the possibility of persistent memory traces. Behavior is a consequence of external stimulus conditions and no assertions are made about intervening, autonomous thought processes. Concept formation, like other forms of learning, is the more or less mechanical connecting up of stimulus and response. No distinction in theory is made between concept formation—the original association of any response with the defining stimulus attributes of a concept—and concept utilization—the reflection of these associations in some different, secondary task such as the identification or discovery of one among several known concepts. Presumably the principles of transfer of training, especially those concerned with stimulus and response similarity variables, are sufficient to an adequate interpretation of "utilization" problems.

"HYPOTHESIS" THEORIES

Hypothesis-testing or process (Kleinmuntz, 1966) theories (no rubric is entirely satisfactory), in contrast to associational theories, typically characterize the concept as some internal, cognitive representation of objective events or relationships among events. Behavior is largely a by-product of selecting a concept as an hypothesis from the available repertoire (from among those concepts which S knows) and acting upon it. The selected concept (hypothesis) is said to "govern" or to control overt behavior (e.g., Restle, 1962). In a problematic situation, S always entertains one or more hypotheses. Each stimulus pattern encountered provides a test of the selected hypothesis—leading to its rejection, revision, or acceptance as a solution. Assumedly the repertoire of available hypotheses at any given time is a function of nativistic factors and past experience. In theory, new concepts are learned or, better, constructed by putting old concepts together in novel ways. When the problem requires the utilization of a known concept, the task is simply to select or identify which of the available concepts is correct.

Unlike their associational counterparts, hypothesis theories attribute important internal monitoring and processing functions to the organism. He receives incoming information, makes some kind of consistency check with his current hypothesis, and undertakes decision processes which result in a modification of the hypothesis and, only secondarily, in an overt act of behavior. Further, reinforcement or informative feedback is presumed to operate not directly on overt behavior but on the implicit processes which control overt behavior (Levine, 1966). These internal activities could, obviously, be construed as the sum and substance of thinking. It might be noted that, although these processes are nonobservable, they are described in terms—reacting to, comparing, selecting and revising, acting upon—much like those used to characterize overt behavior. In some cases, hypothesis theories seem to embody the notion of implicit rehearsal of action, prior to action itself.

INTERNALIZED BEHAVIOR AND THE MEDIATIONAL

What is the origin of an implicit hypothesis or process? Knowing what we do about behavior, the answer (if there is one) must lie partly at least in learning. Surely the individual can internalize only those behaviors which come naturally (e.g., reflexes) or which have been acquired in overt form. Hypothesis-testing and more complicated strategic behaviors are characteristic of sophisticated organisms and depend on a backlog of training and experience which is transferable to new situations.

The mechanics for converting an overt behavior into an internal analogue has been a subject of considerable speculation. A popular and intuitively reasonable argument is the mediation hypothesis, first adumbrated by early Behaviorists, later formalized by Hull (1930), and more recently elaborated in the interpretation of a wide range of behaviors by contemporary S-R theorists (e.g., Goss, 1961; Kendler and Kendler, 1962; Osgood, 1953; Staats and Staats, 1964). The general idea is that in the course of associating external stimuli with overt responses, some representation of behavior becomes anticipatory, producing self-stimulation that has a covert, cognitive or symbolic cue function. The anticipatory behavior might or might not be a full replica of the overt responses associated with the stimulus. It is said to be mediational in the sense that the self-stimulation it produces can become associated with other overt responses as might be appropriate in a subsequent learning situation.

Insofar as conceptual behavior is concerned, the notion of verbal mediators becomes vitally important. Kendler and Kendler (1962; see also Kendler, 1960, 1961) have argued in effect that language, first acquired in the form of overt responses serving primarily a communicative function, becomes the means of regulating other overt actions. As a child matures and learns, his activity is mediated through words. This developmental process is represented theoretically by the transition from a single-unit S-R system to a double- (or multi-) unit system, involving verbal mediational components. The Kendlers' analysis of the change in relative difficulty of reversal and nonreversal shifts in the solution of simple conceptual problems in terms of this transition is well-known. Goss (1961) and Staats (1961; Staats and Staats, 1964) have extended the mechanism of verbal mediation to describe information processing, hypotheses, strategies, and other implicit activities often assumed to occur in conceptual problems.

Somewhat different is a proposal by Mandler (1962). He agrees that the basic form of learning is associational—external S with external R or external R_1 with external R_2 probably mediated by stimuli produced by R_1. With practice (training) the correct response or response sequence becomes stable, able to be run off quickly and without error. In the case of inter-associations between two or more responses, the entire sequence comes to function as a unit, much as the individual response elements prior to training. Once associated (or integrated) the new response unit generates (how is unclear) an internal structural representation or analogue, which functions independently of the overt behavior it represents.

Analogic structures and mediational processes, being the "substance" of covert activity, are two contemporary, associational vehicles of thought. They are said to provide for cognitive control of overt behavior. Many mediators or structures might be elicited in a given stimulus situation—this depends on the history and abilities of S. Overt behaviors are guided by one or another of these internal processes as determined perhaps by their relative strengths, generalization, and other variables. Incorrect or inadequate implicit activities, among those available to S, are rejected (suppressed or extinguished) on the basis of events consequent to overt behavior. Eventually, the correct implicit process will become dominant, relative to others, will be reinforced, and thus will control the characteristics of S's outward performance.

CONTRASTS AND COMPARISONS

It is treacherous to make general statements here, for the individual theorists do not always agree on details. However, there appear to be few substantive differences among the concepts of mediational process, analogic structure and hypothesis. As concerns the analogue and the mediator, two issues arise. First, mediators in general are thought to be associated with overt behaviors, leading directly to the occurrence of those behaviors. There is no implication of similarity in form between the mediational response and the overt response with which it might become associated. Structures, on the other hand, presumably can occur independently of overt behavior and are not associated in the usual sense with a consequent action. However, the analogue (as the name implies) does reflect faithfully and completely some explicit response sequence, to which it leads under the right conditions. Second, the unit mediator seems rather more limited (better defined?) than the unit analogue. Whereas the mediator is described in terms of separable response elements, e.g., attending or naming, the analogue is any functional response unit including those comprised of several integrated sub-units and takes on, though not explicitly, the character of a rule for organizing sub-units rather than the peculiar mechanics of the units themselves. But this latter difference might be illusory, for Goss (1961) has argued that mediators too can be interpreted as complex self-instructions which tell S what to do in an "organized" way.

The hypothesis idea has some of the character of both mediators and analogues. It seems to contribute little in the way of additional insight into covert activity, although the various forms of hypothesis theory might generate unique expectations of data. In general, however, hypotheses are internal representations of external events and/or potential action which very well might arise from the acquisition of behavior in overt form via associational processes. The all-or-none character of concept learning implied by some hypothesis theories (e.g., Bower and Trabasso, 1963), which is said to be a consequence of hypothesis-testing, could just as conceivably reflect the shifting of mediators or analogues.

III

CRITICISM AND AN ALTERNATIVE

Mediating stimuli and responses, symbolic analogues, hypotheses--these are some of the modern counterparts of the older elements of the mind, like ideas, images, and feelings. This is the stuff of which thoughts are made and on which thinking (thought processes?) works its magic—at least so some contemporary theorizing might suggest. Once all the intricate, internal, covert, or hypothetical processes have run their course, out comes behavior. Behavior gets subordinated to the underlying process(es) and begins to take on all the earmarks of a rather trivial, almost unessential thing.

Comment. There is no question about the reality of certain private, implicit processes and events such as subvocal speech or visual images. When the concept of thinking is limited to these covert activities, it takes a weak form and, like overt behavior, is something that a researcher might seek to find out about through experiments. When these (or other hypothetical) covert activities are further endowed with the special properties of antecedence and control of overt behavior, the concept takes the stronger form found in at least some theoretical systems.

WHY THE UNDERLYING PROCESS?

The necessity for assumptions about internal, regulating processes seems to come about from widespread disbelief that overt responses fully reflect all that S has learned or knows. We observe S classifying complex, multidimensional stimulus patterns in a conceptual task, and it is tempting to ask what lies behind it, how did he figure out how to do it, what are the underlying bases of his classification? There are problems, moreover, with analyzing certain empirical outcomes, such as the relative difficulty of reversal and nonreversal shifts (Kendler and Kendler, 1962), in terms of the establishment of discriminative responses to some specifiable, recurrent set of properties of physical stimuli, and so it seems obligatory to assume the existence of preparatory internal activities and to ascribe S's responses to some sort of covert, computational mechanism involving mediators, hypotheses, response generators, or what not. And this is thinking.

CRITICISM

Such theorizing makes for interesting—sometimes exciting—prose; but its contribution to an understanding of thinking and behavior is not always clear. Assertions about underlying processes, and correlative ascriptions of behavior, are not harmless, if they are taken seriously and literally. They demand careful examination and a defense based on necessity, lest the theorist be accused of "explaining away" (not accounting for) important empirical problems. Theories of conceptual behavior which embody assumptions about underlying processes are vulnerable on at least three points.

First, underlying processes are commonly described in behavioral terms. Presumably, they follow the principles of behavior and, in that sense, have all the essential features or properties of real behavior. The process is an invisible behavior, but a behavior nonetheless. In principle, it can do nothing a man cannot do, publically and overtly. The task of accounting for human behavior, then, still remains. In a sense, the theory has simply slipped in an interpolated rehearsal phase between the perception and the overt response phases of an action sequence.

Second, the theory imposes the necessity to study invisible processes—processes which might not exist and for which there is no description (not even physiological) which permits recognition when and if they did occur. There is the familiar argument, of course, that these internal processes are hypothetical and that it really does not make any difference how they are described. One is free to make any plausible identifications between formal parameters of the model and behavioral events. What counts is how well the theory describes the data. But if that is so, why bother with process-talk? It should be possible to describe (and in that sense account for) the behavior of interest, viz., what people can do, without a wistful sojourn into the unknown.

Third, the theoretical underlying process is an invention — an invented answer to the question, what has S learned that enabled him to respond as he did? The invention then is said to explain S's behavior. There is some circularity in this reasoning which is further illustrated in what follows. But the more basic question is whether there is any need to be concerned about a psychological enabling mechanism that permits behavior to be what it is.

None of this should be taken to impugn attempts to describe (or redescribe) conceptual behavior within a particular classification scheme or quantitative model. It is of considerable interest and importance to determine whether performances are aptly described by a small set of strategies, to plot performance changes over trials, to search for rules which characterize response sequences, and to compute the fine grain statistics of data. Whenever empirical information can be summarized in a general equation, it is evidence of genuine progress. These devices provide a handle on behavior--a means for distinguishing behaviors, of recognizing equivalent behaviors and of identifying new behaviors. They help to show how S solved the conceptual problem.

But there is a difference between saying that S's behavior can be described satisfactorily in a particular way and saying that S learned something which allowed or enabled a particular description to be satisfied. For example, it has become routine for mathematical models to give precise and detailed quantitative accounts of learning data. It is largely for this reason that models are of great use and value in experimental psychology. But, as has often been said, a model can be right for the wrong reasons (e.g., Anderson, 1964). Its description can be accurate while, at the same time, untenable psychological properties are assigned to its formal parameters and rules. Neither the conceptual development, nor the use of the model in the analysis of experiments is in any way dependent on these identifications (Bush and Mosteller, 1955). And its value in providing a descriptive account is neither lessened nor enhanced by its psychological "embellishments." Thus, just because S's behavior is accurately "predicted" by some rationally derived equation is no evidence that the underlying processes said to be signified, represented, or identified with the model exist or occur. There are straightforward, objective criteria for deciding whether a model fits the data. But there are no criteria for deciding which, if any, underlying processes enabled the data—S's behavior—to satisfy the model and the description it provides.

Similarly, while empirical findings might be nicely summarized in terms of the rules of performance that S has learned, there is little to be gained by asking what psychological process enabled S to follow the rules. There does not appear to be any way to answer that question without regressing to an invention. And then, there is the problem of recognizing the right answer, assuming that there is one. The only answer that makes sense, considering the derivation of rules, classification systems and models from behavior, has already been given; S learned the solution to a problem, in the course of which he behaved in certain ways which can be described. Manufacturing an answer to the further question about an enabling mechanism (if there is one) is not a condition for understanding or explaining the fact that S did what he did. If the new question and answer routine gives greater significance to that fact, it will be an unusual success.

THINKING AND BEHAVIOR

It would be dead wrong to construe any of this to the conclusion that people do not think or that the phenomena of thinking are not complex. What it means to suggest is that the complexities are behavioral complexities; they reside in what people do or can do, and are not fruitfully relegated to a governing position interior to the organism and antecedent to behavior. The task of psychology is to ascertain which objective factors in the past and present states of the organism and its environment make a difference in how he behaves, so that behavior can be described and predicted rather than merely attributed to a mediational process, an hypothesis selecting and testing device, or some other equally suggestive metaphor (Bergmann, 1943).

How is it possible to say that the complexities lie in S's behavior when in fact all S might be observed to do in the course of solving a conceptual problem is press buttons or sort out cards. These are rather hum-drum, everyday-type responses--easily within the capacities of even some quite stupid organisms. Surely when a human being forms a concept, he learns more than button pressing responses; and it seems fair to ask, what's behind it?

Part of the problem might be that this description is built on an arbitrary and limiting definition of response and of what constitutes a behavior. (Parenthetically, it might be noted that there is an analogous built-in limitation in the common use of the term stimulus as a cue.) The button press (or whatever is required of S on each trial) is typically cited as the functional behavioral unit. References are made to changes in its probability of occurrence and to the possibility of its being reinforceable in a unitary fashion as if it were the response being learned or associated in a conceptual problem. But there is nothing natural about this; it follows more from experimental procedure than from empirical evidence. The selection of a workable unit of analysis does not mean that it is the proper, most meaningful, or most useful unit. Neither does it carry any implications about the structure of behavior or about what is learned. The bounds marking one response from another are often unspecifiable. It is clear that any particular unit, say, a button press, can be redescribed in terms of more elementary components. And, if that is the case, it must also be allowed that any such response might itself merely be a component of an even larger unit. In fact, the whole idea of response units—meaningful as separate and isolated entities—bears careful examination.

But that is not at issue here. The point to be made is that many of the complexities commonly identified with thinking might very well find their representation in behavior—not in underlying, antedating mechanisms—and might very well be observable, describable, and understandable if behavior itself were viewed not as a collection of individual (though perhaps inter-associated) units but as an ongoing rule-following process.

Psychology has been deluged in the last few years (e.g., Miller, Galanter and Pribram, 1960; Smith, 1966; Chomsky, 1957) with demonstrations of the seemingly obvious fact that human beings have an almost unlimited capacity for new, yet systematic behaviors. Actions are so highly organized, even in novel situations, as to defy interpretation based on the prior acquisition of individual responses (even to "similar" stimuli). A common example is the apparent ability shared by all normal people to speak or write such an astronomical number of sentences that it would be impossible in a lifetime to hear them all even once. But almost any example, e.g., solving arithmetic problems, improvising music, or playing basketball, would be equally good. Any definition of learning in terms of associations and arbitrarily small response units gets into trouble with these examples. Such a system does not seem to permit the organism to behave in ways we know he can in fact behave.

What is needed is a description of behavior which allows for its obvious generative and recursive character, which is to say the capacity of organisms to behave flexibly in new situations unbounded by stimulus and response elements common to (or similar with) those encountered in the past. Such a description might well begin with the assertion that what organisms learn are rules and that behavior is a rule-following enterprise. No assumption about conscious awareness or the verbalizability of a rule is necessary; nor is there any need for a commitment to rule mechanisms as an antecedent condition for overt behavior. What is implied is that any particular behavior (response or response sequence) committed by the organism is recognizably consistent with and instantiates a rule. Whether the organism sorts cards, pushes buttons, or says "positive" or "negative" instance in a conceptual task matters little, just so long as he is capable of responding as these task requirements demand. What does matter is that the individual responses are a part of and thereby exemplify what has been or is being learned.

With further elaboration, these statements would surely be classified as "cognitive," for they admit to the possibility that organisms learn, can know, and can understand how to behave. They allow, moreover, that one individual might recognize the rule (or rules) exemplified in the behavior of another. But just as forcefully they eschew the notion of underlying and/or antedating psychological processes which are said to control, govern, or regulate responses. The control and regulation is provided by objective conditions in the situation and by the abilities and skills of the organism.

IV

RELATED RESEARCH

Whether a description of behavior in terms of rules is a viable possibility is something which is unlikely to be settled by any single experiment or perhaps even by all the hard facts that we now have at our disposal. There are, however, a few empirical results which merit attention.

SOLUTION SHIFTS

It is an accepted fact that nonreversal shifts in the solution of discrimination learning and simple conceptual problems are more difficult than reversal shifts for adult human Ss, but that reversals are more difficult for preverbal (and infra-human) Ss. The finding is commonly taken as evidence of the capacity for mediational processes (antedating overt behavior) in the verbal organism. This finding, however, is not as stable as might be hoped. For example, if the adult S's attention is drawn by instructions to the various stimulus dimensions, with the admonishment that any one but only one is correlated with the correct category responses, the difference in difficulty vanishes; the two types of shift are accomplished with equal rapidity (Johnson, Fishkin, and Bourne, 1966). Moreover, with repeated shifts, in a learning set paradigm, the initial difference in difficulty is eliminated. Both types of shift are made in a minimal number of trials; and this is true whether S is an adult human being (for whom the reversal is easier to begin with), a young child (for whom the nonreversal is initially easier)[1] or a member of some subhuman species (Dufort, Guttman, and Kimble, 1954; Schusterman, 1964). What are recorded in these experiments are the individual, trial-by-trial (category) responses of Ss and it is natural to think of them as being learned (or associated) and to deal with them as the data to be "explained." The general findings, however, might suggest that these responses merely instantiate what has been learned—that S has learned to behave—within the physical limits and requirements of the task—in accord with a rule. Adult human beings often can state that rule verbally—when the solution changes (and one error can signal that) find a new stimulus characteristic which is correlated with correct responses; often there will be enough information provided on the error trial to determine the new solution. But the ability to verbalize is no condition for using the rule. The rule is a way of describing and identifying what S is doing, and is neither cause nor explanation for that behavior. Why there is a transition from nonreversals as the easier of the two tasks when no pretraining or instructions are given is more difficult to account for. It is an interesting, though fragile, empirical fact which might have something to do with the prior learning of stimulus dimensions (Riley, McKee, and Hadley, 1964), the concept of opposites (Bogartz, 1965; Kroll and Schvaneveldt, 1965), or some other peculiar characteristic of the history of human beings. But it, too, compels no assumptions about underlying, behavior-controlling processes.

LEARNING SIMPLE LOGIC

More to the point perhaps is some recent research on the learning of relatively simple logical operations. Class concepts can be defined by a variety of operators, among which are the conjunctive, "x and y," the disjunctive, "x and/or y," the conditional, "if x then y," and the biconditional, x if and only if y," where x and y are any two unique stimulus attributes. In these experiments, Ss learned a variety of class concepts within a task format similar to the conventional concept-learning paradigm. Specifically, Ss learned or dis-

[1] This conclusion is based on data collected by P. J. Johnson and J. Wadsworth at the University of Colorado. Details of the study will be reported elsewhere.

covered how to sort stimulus patterns into categories (positive and negative instances of some unknown concept) by observing the proper placement of a series of examples. The problems were of two types: (a) S might be required to learn the defining relationship between the relevant attributes of a concept; under these circumstances, the attributes were named at the outset and S determined how they were combined, "x _?_ y," to specify the concept. (b) Alternatively, S's task might be to identify the relevant attributes; here the relationship was given (through instructions and pretraining, if necessary) and the problem had the form "? Q ?," where Q is some chosen connective, such as conjunction.

Connective Learning

A number of experiments, contrasting attribute and connective learning problems and comparing the various connectives for difficulty, have been conducted (some of which are reported elsewhere, e. g., Haygood and Bourne, 1965; Bourne, 1966c). The S's behavior exhibits some interesting and unique features which are best shown by means of illustrative results. Without any pretraining, some Ss were given the task of learning to sort geometrical designs in accord with the concept "x _?_ y," where the blank, unknown to S, was if and only if. There were 81 geometrical designs in total, the population being generated by four dimensions (color, form, number, and size of figures) each with three values. As a concrete example, let x and y be the attributes redness and triangularity, respectively. The concept then, "red if and only if triangle," required that all patterns which were red and triangular and all patterns which were neither red nor triangular (e.g., blue circles or green squares) be sorted into the class of positive instances and that all red nontriangles and all nonred triangles be labeled negative instances. The required arrangement is shown in Table 1.

On the average, naive but relatively intelligent human beings took about 60-70 trials (different examples) to solve this problem. Over the course of these trials, they responded to and observed the correct placement of several patterns of each type, i. e., red triangles, nonred triangles, etc. The probability of an incorrect response by S on these trials is shown in Table 1—under the heading, Problem 1—for all four types of instances. A more detailed breakdown shows these probabilities for the first and second (or first through fourth) examples of each type. (We limit ourselves here to different examples, of which there were only two for nonred trian-

Table 1

Description of the Stimulus Patterns, the Stimulus Classes, and the Results of a Connective-learning Experiment

Stimulus Classes	Illustrative Description	Response Class	Overall Error Rate	Problem 1 Ordinal Position	Problem 1 Individual Error Rate	Problem 7 Ordinal Position	Problem 7 Individual Error Rate
TT	RTr	+	.22	---	---	---	.11
TF	$R\overline{Tr}$	-	.43	1st	.51	1st	.51
	(RC, RS)			2nd	.44	2nd	.00
FT	$\overline{R}Tr$	-	.39	1st	.48	1st	.48
	(GTr, BTr)			2nd	.40	2nd	.12
FF	$\overline{RTr}$	+	.68	1st	.82	1st	.49
	(GC, GS,			2nd	.77	2nd	.17
	BC, BS)			3rd	.69	3rd	.00
				4th	.73	4th	.00

Note: The following abbreviations are used: T, true (or present), F, false (or absent); R, red; G, green; B, blue; S, square; Tr, triangle; and C, circle.

gles—green and blue triangles—and only four for nonred nontriangles, as illustrated in Table 1.)

Errors (misplacements by S) were distributed unevenly over the various types of patterns; Ss made fewest errors (both proportionately and in absolute number) on red triangular patterns (recall, S had been told that redness and triangularity were the relevant attributes) and most errors on nonred nontriangles (which, like red triangles, were positive instances). Probability of error showed a small, but reliable, decrease from the first to the second (or fourth) example of each type of pattern.

Another group of Ss was given precisely the same task, after a pretraining routine during which they solved a series of six connective-learning problems. Each pair of pretraining problems involved a different connective, i.e., two conjunctives, two disjunctives, and two conditionals—but of course none of these was identical to the test connective, a biconditional. And, of course, the two relevant attributes were different for all seven problems. The order of connectives during pretraining was counterbalanced within the group.

On the average, these Ss required 5.5 trials to attain solution, i.e., to achieve errorless performance on the biconditional problem. Mean error probabilities for the various stimulus patterns are shown in Table 1, under the heading Problem 7. Several features of the data are notable. First, the majority of Ss (77%) solved the problem with at most one error on each of the four types of instances. This precludes the computation of meaningful error rates for the four types of patterns. It can be seen, however, that the probability of an error on the first example of each type was about the same, .5. One can safely ignore red triangles, for those instances containing both relevant attributes were positive under all the connectives considered here; thus S was likely to guess positive for red triangles in Problem 7. The difficulty associated with placing nonred nontriangles in the positive category (along with red triangles with which they share no common attributes) was all but eliminated with pretraining. Finally, the probability of error on the second example (recall, it had at least some different stimulus attributes) of any type was virtually zero. On the third example, and all thereafter, it was zero.

These data imply that Ss' behavior with training comes to be organized in a particular way. It is as if Ss learned an algorithm in any problem of this type, observe the correct placement of an example of each of four classes of patterns distinguished by the presence or absence of the two given attributes; then classify all subsequent examples according to these four observations. One could go further to say that S's behavior looks as though it is mediated by something akin to a truth table. The designations TT, TF, FT, and FF for the four stimulus classes are used in Table 1 to emphasize this possibility. Clearly proposition "$x \; ? \; y$" is fully determined by establishing the truth value of xy, $x\bar{y}$, $\bar{x}y$, and $\bar{x}\bar{y}$ (or of TT, TF, FT, and FF instances). So maybe what S has learned or discovered is a deductive device like that used in elementary logic, which enables the overt performance.

The S does behave in accord with "truth table" rules. But this in turn does not imply that his overt behavior is mediated by an antecedent "look" at an implicit truth table (or any representation thereof). Subjects do not report any systematic prior rehearsal of overt behavior. Further they have a difficult time verbalizing the structure of their behavior or any correlative implicit responses. Subjects behave as they have been trained to behave. The complexities of their action resides in the behavior itself, in the manner in which it is "run off" and not (at least not necessarily) in any preliminary covert representation of behavior. The Ss were trained to solve connective-problems, and solve them they do when the necessity is imposed. What they have learned is a complex behavior which can be described as a rule. And there seems to be no point to inventing a psychological mechanism that enables S to follow that rule.

Remark

Some of the "fine grain" features of category responses might be noted in passing. It is tempting to say that on Problem 1 Ss seemed to be learning gradually. Correct response probabilities increase over successive presentations of instances of the same class, as might be expected if associat[illegible]s were being formed. On the other hand, the learning in Problem 7 looks all-or-none. The error rate is roughly .5 until S makes his last error, after which it drops to zero. Which set of assumptions about underlying processes—incremental association formation or all-or-none hypothesis testing—gains the greater support from these data? It is difficult to decide on an answer to that question. Still S's behavior makes sense if one looks not only at the response on each trial but also considers the way in which these responses fit in as a part of S's ongoing, rule-following behavior.

Attribute Identification

All (or nearly all) Ss learn, with training, to solve in an efficient way conceptual problems in which the connective is unknown. There is additional evidence of a similar behavioral "strategy" in attribute identification tasks. With experience in solving these problems—rule given, attributes unknown—Ss act as if they know and understand the truth-table (not really a surprising result) and can identify the unknown attributes in exactly four instances if these instances are chosen properly to represent the four stimulus classes. While familiar enough to academic-types, the problem-solving process is relatively complex. Still the complexity is there in S's behavior—open and visible to the public, and not hidden away in some implicit form.

Logic Pretraining

One other experiment might be summarized. The results so far suggest that preliminary instructions (or alternatively, practice) in constructing truth-tables ought to facilitate the learning of class concepts. Desiring not to be quite so direct about it, Haygood and Kiehlbauch (1965) pretrained Ss to sort geometric designs into four categories defined by the presence or absence of two named attributes. For example, given redness and triangularity as the attributes, S learned to place red triangles (TT) in one category, nonred triangles (FT) in a second, red nontriangles (TF) in a third, and nonred nontriangles (FF) in a fourth. Four sorts of this type were required (each with a different pair of attributes) after which S solved one experimental problem of the connective-learning variety. For different Ss, the unknown connective was conjunctive, disjunctive, conditional or biconditional. Other Ss solved the same problems without this form of pretraining. Mean number of trials to solution reqiired by pretrained Ss were 6, 6, 11, and 22 for the four connectives in the order given above. For nonpretrained Ss, the comparable means were 12, 29, 32, 51. The difference was highly reliable. These results take on added significance if it is understood that sufficient information to solve each problem is given in the first six trials (on the average). Thus, Ss pretrained to categorize in accord with the rows of a bidimensional truth-table solved conjunctive and disjunctive problems with maximal efficiency. The apparent difficulty with conditionals and biconditionals can be assigned with some confidence to conflicting extra-experimental experience; both connectives require, for example, that FF patterns be grouped with TT patterns as positive instances.

In passing, it might be noted that related results have been obtained from children, ranging in age from 4.5 to 7 years. This study was limited to conjunctions and disjunctions, on the assumption that young people might find the other connectives too difficult. As it turns out, this was probably a miscalculation. Again, there was a marked facilitative effect of experience with the four-class sorting task. The effect interacted with age, with 4.5-5 year olds seeming to benefit only slightly (about 17% reduction in trials to solution with pretraining) while 5.5-6 year olds (about 60% reduction) and 6.5-7 year olds (about 77% reduction) derived considerable benefit. The oldest Ss displayed near maximally efficient problem solving on both conjunctions and disjunctions, their data being quite similar to those of college students.[2]

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[2]These results are drawn from a study conducted by P. J. Johnson and Anne Fishkin at the University of Colorado. The experiment will be described in a separate report.

V

FINAL REMARKS

The main question seems to have been lost. What does research on conceptual behavior tell us about thinking? What has been said up to now might be taken as a denial of thoughts and thinking altogether. Since that was not the intent, some attempt at clarification is necessary.

People do have thoughts. Moreover, people do engage in implicit, covert activities, like subvocal speech. These things seem self-evident; at least nothing that has been said heretofore was meant to question them. What has been questioned is the notion that implicit action regulates or that thinking "explains" overt behavior. Both implicit and explicit behavior—and thoughts, too, for that matter—are conceived to be part of human skills and abilities, to be described and understood as performances. The assumption that one, as a hidden computing process, regulates and results in the other is rejected.

Saying this, of course, neither makes it so nor makes the job of understanding thought and/or implicit behavior any easier. At the most, it can only help to see why theory and research in concept attainment is unlikely to make any special contribution to this understanding. Nothing changes the fact that thought (as a psychological concept), whatever else is involved, is intangible, private, and without substance. This is what makes description difficult, allows speculation to flourish, and generates a good deal of confusion.

WHAT IS THOUGHT?

There is no universally satisfying concept of thought. About the only thing that can be said with any confidence is that thoughts are events. Like all events, they are datable and locatable (i.e., they occur in time and to a person) and they reflect change. The subject of change is, of course, the person to whom the thought occurs; but the more penetrating question is, can anything precise be said about the difference in his state before and after the thought? Several answers might be attempted, implicating consequence implicit activities, such as verbal or visual imagery or behavioral dispositions and correlative physiological processes. But about the thought itself, only one answer seems to satisfy all sensible cases—the thought is a change in the individual's position with respect to other things, i.e., persons, objects, events, etc., which can sense and detect; in a word, it is a change in the individual's possibilities (or potential) for behaving (Ossorio, 1966).

To illustrate, if I think of a solution, or a possible solution to a problem, my relation to that part of the world has surely changed. I can now perform, i.e., solve the problem or attempt to solve it, whereas before I could not.

A thought is said to be a change in state. And the resulting state is a position from which the individual might undertake action. In that sense, then, the thought is describable as a state of readiness (incipient action?); behavior can follow naturally, and the thought is part of that behavior. It must be clear that behavioral sequelae can be either explicit—an overt attempt at proper performance—or implicit—as, for example, in subvocal speech. But to recognize implicit behavior is not to endow it with the special property of control over overt attempts. Note also that a thought might have no behavioral consequences if, for example, one change of state follows rapidly upon another.

One final comment: if thoughts are events, why isn't thinking a process, a mental process buried away in the deep structure of behavior? If behaviors such as subvocal speech are classified as thinking, then thinking is a process. But it is a process that follows from thought, rather than something which is likely to produce thoughts. More commonly, the thought process is imagined to be something called calculating, pondering, reflecting, deliberating, etc., which has as its outcome a thought.

Whether there is some such process is indeterminate. No matter what else might be said, it is easy to see that what is at issue here is hardly the same concept of process fruitfully used in other sciences, for psychological processes leading to thoughts do not occupy any definite time interval, nor are they identifiable independently of an initial state or an outcome (Ossorio, 1966). Examples of scientifically useful processes are plentiful—flow of water, heat transfer, growth in plants, and so on. But having a thought, making a decision, calculating a sum, selecting an hypothesis, or forming an association, as processes, lack determinable time characteristics and descriptions which are independent of initial condition and outcome. (What would it be like to be half-way through the process of having a thought or making a decision?) The problem can further be characterized by saying that it is not so much a matter of lacking information or of being unable to observe the process as it is simply not knowing what to look for. It is a search for something only rumored to exist, but so indescribable that we can't even tell when one occurs.

So what is missing in an account of behavior which fails to mention underlying processes? There are physiological processes, which hopefully can be related empirically to thinking and behavior; and there are behaviors, which might be liberalized to include thoughts as changes in potential for action. These things can be described. Is there anything more? Are there real questions that can be asked about thinking and behavior whose answers depend on the discovery of an underlying, psychological controlling process? Real questions exist only when there is a way to recognize the right answer. And questions about thinking and behavior can probably be answered recognizably in terms of describable experience, ability, and performance.

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